THE object of my letter to which Prof. Cox refers was to draw attention to certain statements made in recent accounts of Arrhenius' theory which were disproved by Prof. Schwarzschild's computations. I was fully aware at the time that Arrhenius himself had already arrived at the conclusion that, to accord with his theory, the particles in the tails must be assumed to be liquid or solid. This was the necessary result of his computations, which had convinced him that the diameters of the particles must be between 0·1 and 6μ in order to satisfy Prof. Bredichin's values for the repulsive forces observed in comets. But how does Arrhenius' theory account for the presence of luminous vapours in the tail? In some recent comets the typical spectrum of the hydrocarbons was traced by Prof. Vogel to the farthest end of their tails. The emission of Comet 1881 iv. (Schaeberle) was almost entirely gaseous, and in Comet 1882 ii. even the sodium vapour was observed in the brighter parts of its luminous appendage. How are these vapours carried into the extreme parts of the tail, since the analysis of Prof. Schwarzschild shows that the pressure of light is far too insignificant to exert a repulsion upon the molecules of a gas or vapour?

Prof. Cox assumes the evaporation on the side facing the sun to be caused by the "intense heat" to which the comet is exposed on its approach towards our luminary. He thus attempts the revival of an hypothesis now abandoned by astrophysicists. Astronomers will find it somewhat difficult to comprehend how, for instance, the famous comet of 1811—one of the most remarkable phenomena of last century—which never approached the sun to the distance of our planet, could have received so intense a heat-supply that the "hydrogen of the hydrocarbons would boil off." But apart from this, the spectroscope has now clearly demonstrated the luminosity of the cometary substance to be due to disruptive electric discharges at a low temperature. The assumption of an "intense heat" causing the evaporation on the side towards the sun receives no support from the spectroscopic evidence. Moreover, the misty film surrounding the nucleus, the so-called atmosphere or coma, is certainly of extreme tenuity, and the mass of the comet cannot but be immeasurably small. Hence the hydrostatic pressure opposed to the outpouring vapours must be extremely insignificant. This necessarily involves low boiling points, so that condensation can only take place at very low temperatures. Hence we require at the same time an intense heat to boil off the hydrogen and an extremely low temperature to allow the condensation of the hydrocarbons into drops.

But even suppose that in spite of the intense heat and the low hydrostatic pressure condensation does take place on the side towards the sun, and that drops of hydrocarbons with less than the "critical" diameter are driven from the sun by the pressure of light. Can Prof. Cox demonstrate the possibility of these drops preserving their liquid state after having been launched into the vacuum of space? The permanent existence of drops of hydrocarbons in the tail is possible only under the condition that the space between the drops is saturated with hydrocarbon vapours. Here, then, we are again confronted with the question, How are these vapours carried into the tail, since the pressure of light has practically no repulsive power on the molecules of a gaseous substance? But if only drops, and no vapours, of hydrocarbon are repelled by the light-pressure, as Arrhenius assumes, what force prevents these drops from being instantaneously evaporated after once having departed from the outskirts of the comet's atmosphere and having started on their journey through the vacuum of space? Obviously the assumed drops ought to retain their initial bulk throughout the whole length of the tail, *i.e.* through a distance of hundreds of thousands of miles, all the time unsurrounded by any vapour, the tension of which might counteract the inherent tendency of the liquid to assume the gaseous state. Such an hypothesis is plainly impossible.

Prof. Cox mentions the possibility of solid particles being repelled by the light-pressure. He remarks that "ultimately solid carbon is thrown out, finely divided as in an ordinary flame." There is no objection to this assumption from the physical point of view. But is it sufficient to explain the characteristic forms of the tails and their classification into several distinct types? What reason can be adduced for particles of dust assuming only such dimensions as would lead in all the comets to only three or four particular repulsive forces out of an infinite number of possible varieties? Why have, for instance, in the forty comets investigated by Prof. Bredichin, the particles never assumed such dimensions as would correspond to types intermediate between Bredichin's first and second? The explanation of Prof. Arrhenius is very unsatisfactory. He says:—"Wenn nun zufolge gewisser Umstände einige Tropfengrössen die gewöhnlichsten sind, so können die wohlbekannten, relativ scharf begrenzten Schweife von verschiedener Krümmung entstehen." No attempt is made in his paper to show what these "certain circumstances" are, nor why they should lead to the same types of tails in comets with widely different conditions of evaporation and condensation. The results of Bredichin seem to me indeed to be irreconcilable with the present version of Arrhenius' theory, which in no way explains the remarkable selection of repulsive forces discovered by the distinguished Russian astronomer.

Another difficulty has been pointed out in my previous letter. It relates to the peculiar behaviour of the coma. In some comets a contraction of the coma has been observed on the approach of the comet towards perihelion, succeeded by an expansion after the perihelion had been passed. Thus the diameter of the coma of Encke's comet in 1838 was found by Valz to have shrunk from 280,000 miles at the solar distance 1·42 to only 3000 miles at perihelion. How is this phenomenon to be explained by the pressure of sunlight? In many instances (Comet 1862 ii., Respighi's and Henry's comet among others) the coma retained its globular form while the tail spread out and assumed enormous dimensions. But the spectroscope has now demonstrated that no difference exists between the coma and the tail with regard to the physical and chemical constitution of their materials. Hence the question remains still open why the pressure of light should repel the materials of the tail and yet at the same time leave the same materials in the coma entirely unaffected.

My objection to the theory of Arrhenius refers to those parts where he introduces Maxwell's pressure of light. I am perfectly at one with the Swedish physicist and with Prof. J. J. Thomson regarding the important part probably played by the negative electrons emitted by the celestial bodies. But I fail to understand why the pressure of light should be required to account for the discharge of negative electrons into space. Physicists tell us that a hot body like our sun is most probably the source for an energetic emission of free electric atoms. We are, moreover, acquainted with the fact that these free electrons possess enormous velocities. The measurements of Wiechert have shown the velocity to be between one-fifth and one-third of that of light. Now if the heat of the sun is capable of splitting off the negative electron from its atom, a great number of these free electric atoms must be flung into space simply on account of their enormous kinetic energy. For no form of matter leaving the upper strata of the solar atmosphere with a velocity exceeding 600 kilometres per second can possibly return to the sun. Why, then, should the free negative electron, with more than one hundred times this critical velocity, still require a force such as the pressure of light to be propelled into the universe? If we adopt Arrhenius' idea, according to which the free electrons first condense ordinary matter around themselves near the solar surface and are afterwards driven off by the pressure of light on this bulk of matter, we must find it difficult to understand how in some authenticated cases the action of a solar outburst on the magnetic instruments could have been instantaneous (see Young, "The Sun"). Granting the highest possible repulsive action of light-pressure on small particles, the solar electrons would require at least sixteen hours to reach the surface of our planet.

In my opinion, if we adopt the suggestion of Prof. J. J. Thomson that free negative electrons are probably emitted by the sun, a copious propagation of these infinitesimal corpuscles into space would be the obvious and necessary result of such an emission, even without the assumption of light-pressure.

The train of reasoning ensuing from this hypothesis would lead in a most natural way to Zöllner's celebrated theory of comets. By the abundant presence of electrons, space has then to be considered as a negatively charged electric field acting upon the *ionised* cometary matter. From this point of view, Zöllner's theory—according to Newcomb, the one "which on the whole most completely explains all the phenomena"—would no longer "lack the one thing needful to accept its reception," namely, "the evidence that the sun acts as an electrified body."

The main conclusion I have arrived at after a careful study of the theory of Arrhenius amounts to this: that by abandoning the assumption of the pressure of light and by assuming the propagation of free negative electrons from the sun into space

simply as a consequence of their great inherent velocity, the theory becomes admirably fitted to strengthen the views of Olbers, Zöllner and Bredichin with regard to the nature and the origin of the repulsive force acting upon the cometary matter. But the introduction of Maxwell's pressure of light gives rise to a number of difficulties which, as Prof. Arrhenius abundantly shows, can only be overcome by arbitrary and unwarranted assumptions.

I shall take an early opportunity of demonstrating the superiority of Zöllner's theory over the one which now claims to "sweep the astronomical horizon of so many mysteries."

Royal Observatory, Edinburgh. J. HALM.

Stopping down the Lens of the Human Eye.

MAY I be permitted to direct Mr. Wm. Andrews' attention to the fact that his experiment in "stopping down" the lens of the eye involves exactly the same principle as "orthoptics," of which every rifle-shot will have had experience.

The "orthoptic" consists of a round hole in a black disc, which replaces the lens of a pair of spectacles. The hole is generally adjustable in size, to suit varying conditions of light. The purpose of the orthoptic is to increase the *depth* of focus, enabling both back and fore sights and the target to be in sharp focus together. Persons with naturally large pupils will, as a rule, notice the effect more strongly. H. BLISS.

May 9.

IT may interest your readers to know that the principle referred to, under the above heading, in your issue of May 8 was adopted, a great many years ago, by the late Lord Sherbrooke, whose sight I believe was very defective. I remember seeing, about the middle of the seventies of the last century, at an exhibition of physical apparatus at South Kensington, a pair of spectacles which were said to have been invented by him for his own use. They consisted of two convex metal cups, closely resembling in shape and size the bowl of an ordinary tea-spoon. In the centre of each cup was a small pin-hole, which was the only aperture through which light could enter; and the two cups were fastened together by an elastic string, evidently intended to go over the head. The invention impressed me at the time as a remarkable example of scientific skill combined with great simplicity of contrivance.

GERALD MOLLOY.

The Evolution of Snails in the Bahama Islands.

IT seems desirable to call the attention of evolutionists to Dr. H. A. Pilsbry's monograph of the genus Cerion (or Strophia), just published in the "Manual of Conchology." The facts presented are most of them not new, but all that is known is set forth in great detail, with an abundance of excellent figures. Cerion is a genus of rather large cylindrical land-shells, for the most part inhabiting the Bahamas and Cuba. It has split up into innumerable local species and races, 134 of which are recognised as sufficiently distinct to bear names. Not only do even the smallest islands or "keys" produce distinct species, but frequently one small island will have two or more different forms inhabiting different parts, and sometimes a distinct race will occupy a very small area, surrounded on all sides by another type. The problem of the differentiation of the Achatinellidæ in the Hawaiian Islands is complicated by the complexity of their environment; but here in the Bahamas we have differentiation just as marked, with an environment—small sandy islands with palms and low bushes—as simple as we are likely to find anywhere. It would therefore seem that an excellent opportunity lies before some student of evolution to investigate exhaustively the local species and races of these Bahama snails, and determine what causes have brought about the known results. Colonies could be taken to new localities, and watched from year to year to see whether they became modified. The food and moisture conditions might be altered, and the results observed. The exact conditions surrounding each distinct form might be studied and described. Thus it might be determined whether the differentiation was the result of natural selection or has taken place independently of it. Such an investigation would be delightful work for some enthusiastic naturalist, especially with such an excellent guide in hand as Dr. Pilsbry has supplied. T. D. A. COCKERELL.

East Las Vegas, New Mexico, U.S.A., April 26.

Retention of Leaves by Deciduous Trees.

THE retention of leaves by beechen hedges referred to by your correspondent in NATURE, April 10, is by no means confined to those on elevated ground. It may commonly be observed in hedges of this tree whatever their situation. In Northumberland the beech is not infrequently used as a hedge, and always retains its leaves throughout the winter. Young beech trees also frequently retain their leaves, and by no means always in exposed situations. Indeed, the examples I have myself seen have been much more frequently in sheltered spots, as in plantations of older trees.

Nor is this phenomenon of deciduous trees retaining their leaves under certain conditions confined to the beech. It is, perhaps, equally common in the oak. Young oak trees in plantations may often be seen in the spring covered with brown and withered leaves. Larger trees may also sometimes be seen retaining the leaves on some of the *lower* branches, while the *upper* ones are bare. Travelling from Eastbourne to Victoria, soon after reading the above communication in NATURE, I noticed hundreds of young oaks covered with withered leaves. None of these were in elevated or exposed situations. Indeed, I am inclined to suggest, as an inference from the above facts, that it is rather the *protection* enjoyed by the trees which *enables* them to retain their leaves. In the one case the lesser height of the tree, and in the other the close intergrowth of the hedge, gives the wind less power to strip off the leaves. We can hardly consider that there is here a "protective device," unless on the part of the gardener who sets a beechen hedge to shelter his plants. G. W. BULMAN.

13 Vicarage Drive, Eastbourne, May 3.

WITH regard to the interesting communications concerning the retention of their leaves by young beeches, I beg to forward another possible solution. The beech is a "frost-tender" species, and early frosts, which would not rise high enough to affect large trees, would freeze the leaves of "small young" trees, thus preventing the formation of the abscission layer of cork at the base of the petiole. In such a case there is no reason why the leaves should fall off for a considerable time.

Leaves killed before the formation of this layer remain on the branch for an indefinite time, of which phenomenon pea-sticks cut in full leaf may serve as an example. P. T.

May 10.

THE RECENT VOLCANIC ERUPTIONS IN THE WEST INDIES.

NEWS of the terrible volcanic eruption in Martinique reached this country on Thursday last, and the details which have since become known have shown that an appalling disaster has occurred. St. Pierre, the chief commercial centre of the island, has been totally destroyed, and about thirty thousand people have perished. The eruption of Mont Pelée began on the night of Saturday, May 3, when large quantities of scoriæ and volcanic ash were thrown into the surrounding country. On Monday, May 5, a stream of lava is reported to have rushed down the side of Mont Pelée, following the dry bed of a torrent, and reaching the sea, five miles from the mountain, in three minutes. When the stream met the sea the water receded 300 feet on the west coast, returning with greater strength in a large wave.

Two days later, on May 8, a similar torrent of incandescent lava engulfed the town of St. Pierre. The following telegram describing the calamity was received at Paris from Fort de France on May 11, and was published in Monday's *Times*.

"The town of St. Pierre was destroyed on the 8th about 8 a.m. A terrible torrent of incandescent lava, from Mont Pelée, a volcano a few kilometres from the town, accompanied by a shower of fire, in a few seconds covered the town, and an immense furnace extended over the neighbouring coast, thus forming a line of fire from the village of Carbet to the town of Prêcheur. The effects of this volcanic torrent were felt as far as